CORRESPONDENCE.

Contributions to the Pathology of the Blood. By PROF. VON S. STRICKER. Translated from the German by F. J. BUMSTEAD, M.D.

[IN the American Journal of the Medical Sciences for January, 1868, Prof. J. H. Salisbury, of Cleveland, Ohio, published a "Description of two new algoid vegetations, one of which appears to be the specific cause of syphilis, and the other of gonorrhœa."

The subject attracted but little attention until on the 26th of January, 1872, when Dr. Adolph Lostorfer (see *Archiv. für Dermatologie und Syphilis*, IV. Jahrgang, erstes Heft, 1872), read a paper before the Wiener Gesellschaft der Aerzte, a paper, "*Ueber die specifische Unterscheidbarkeit des Blutes Syphilitischer*," which, for the moment, excited on the part of many of the members a furore of applause, but which met with much subsequent opposition and gave rise to discussion of a violent and personal character.

Prof. Stricker, whose pupil Dr. Lostorfer was, then undertook a series of investigations upon the subject, at which he kindly invited me to be present. The results are given in the following pages, which I have translated from advance sheets of the *Medicinische Jahrbücher*, furnished for this purpose by Prof. Stricker. It is not inappropriate that they should find a place in the Journal which first published Prof. Salisbury's paper, although no one, I presume, after reading the two papers, will be inclined to believe that Prof. Salisbury's "crypta" and Dr. Lostorfer's "corpuscles" are one and the same thing, and Prof. Stricker does not regard them as identical.

But however this may be, it is fortunate that the fungous origin of syphilis has been so thoroughly investigated by such a keen and able observer as Prof. Stricker, and so far as the physical elements of syphilitic blood are concerned, it is safe to say that they are here described as fully and as perfectly as the best microscopes of the present day will enable us to appreciate them. F. J. B., Vienna, May 12, 1872.]

I undertook the following investigation to decide two questions; I desired in the first place to ascertain the nature of the corpuscles discovered by Lostorfer, and in the second to know, from my own examination, whether these corpuscles, as Lostorfer had asserted, were peculiar to the blood of syphilitic persons.

I commenced my investigations with the blood of three syphilitic men, and in all three the corpuscles in question were found in abundance in the course of a few days; I therefore supposed that I had obtained abundant material for the solution of the first question.

A preliminary inquiry was, whether Lostorfer's corpuscles are not present in fresh blood. Before answering this, I will describe the mode in which I prepared and kept my preparations.

In preparing specimens of blood merely for examination in a fresh state with the highest powers of the microscope, I endeavour to make them as thin as possible. The slide and cover glass should first be carefully cleansed and dried. A small puncture is made in the integument of the person whose blood is to be examined, from which the blood will ooze forth. The covering glass is now held by its parallel edges between the thumb and forefinger, and its central portion is brought in contact with the apex of the drop of blood emerging from the wound, so that a minute portion adheres to the surface of the glass. If the covering glass is now quickly applied to the slide, the minute drop of blood will, in most cases, spread itself out sufficiently; otherwise, a gentle pressure upon the covering glass is required. Since the examination is to be carried on with immersion lenses, and since it is of importance to be able to examine the specimen near to its edges, while, on the other hand, any overflow of water beyond the sides of the covering glass spoils the specimen, it may not be superfluous to remark that I apply the water, required by the immersion lens, only in very small quantity and to the upper surface of the covering glass by means of a pipette, drawn out at both ends to a very fine point.

The appearances to be seen in fresh blood are manifold. In the blood of many persons, to be sure, only the well-known elements are found, to wit: red blood corpuscles, colourless blood cells and smaller, colourless nucleoli. Very frequently, however, there appear in larger or smaller numbers colourless bodies from the size of a nucleolus to that of a colourless blood corpuscle. These are especially visible when working with very high powers.

It would be fruitless to describe the varied forms of these bodies. I will only say that many of them appear as if they were fragments of young cells, whilst others give the impression of being knotted points of coagulation. The latter are chiefly found in places where the preparation is somewhat thick, and where the interspaces between the blood corpuscles are traversed by numerous fine threads.

It is not improbable, therefore, that the branched, knotted points, as well as the fine threads, consist of fibrine. Those bodies which look like portions of colourless blood corpuscles are, like the last mentioned, various in form and seldom spherical.

Wherever a rich net of fibrine is present, numerous and exceedingly fine particles are to be found, but it is easy to convince one's self that they are for the most part sections of fibres running parallel or obliquely to the axis of the microscope.

Besides the above mentioned constituents, very minute particles are seen in the blood of many persons, which can scarcely be defined by an immersion lens No. 10, and which are frequently in active oscillation. Whether these oscillations are exclusively Brownian, or whether the particles are sometimes organisms showing vital movement, I will not attempt to decide. The question, however, appears to me worth raising, in order to call attention to the fact, that the existence of very small organisms of a low order in the circulating blood of healthy persons is not excluded.

With regard to the presence of such foreign bodies as accidentally get mixed with the preparation, any one conversant with the microscopical examination of the blood, can decide.

In rare instances, however, corpuscles are found in fresh blood, with regard to which it is difficult to say whether they are an accidental admixture, or if they were previously present in the circulating fluid. They are, for the most part, spherical, with a dark outline, and they are distinguished by the fact that when seen under a very high power (objective 15 of Hartnack) and with sharp adjustment, they are very dark.

For the purpose of breeding the corpuscles which we now have in view, we have to preserve a good specimen of blood under exposure to the air, for as long a time as possible. This object will be attained if the preparation be kept in a chamber in which it is on the one hand protected from drying, and on the other hand from moisture. That the drying of the preparation is not advantageous, is self-evident. The same is almost equally true of any thinning of the blood through the addition of moisture. The corpuscles of the blood are very susceptible to any thinning of the plasma in which they float. The red corpuscles become pale; the plasma takes up colouring matter and becomes turbid. I will hereafter show that the gradual addition of water is highly prejudicial for the purpose we have in view. I can only state in general terms how these two conditions, viz., preventing the specimen from drying, and also keeping out water, are to be attained. Every experimenter must determine the finer shades for himself. Place then the specimens in a chamber in which the surface of water therein contained shall be small in proportion to the size of the chamber. For an exsiccator glass, such as chemists use for drying objects, a moist sponge the size of a walnut is sufficient to protect the specimens from drying. Beneath this glass I place a framework that will hold about a dozen specimens. If the slides are labelled, specimens from various sources may be placed in the same chamber and upon the same frame.

In order to favour the development of the corpuscles, I prepare the specimens somewhat thicker than above described. Before collecting the blood, I warm the slide and smear upon it two thin strips of wax in such a manner that the covering glass can rest upon them. After the specimen is prepared I press the covering glass down upon the wax until the rolls of adherent blood corpuscles are spread out in the form of a net. The meshes (plasma-islands, or interspaces) of this net, filled with plasma, afford breeding spaces which are admirably adapted for our purpose.

It is desirable, for reasons to be given hereafter, to make the specimen, relatively to the covering glass, so small that its edges may be examined with an immersion lens.

When the specimen is thus put up, it is also easy to test microchemically the granules which are found in the plasma-islands. The preparation of blood is, in fact, transformed into a comparatively firm cake by means of the threads of fibrine which run through it; the granules themselves are often seated in a network of fibrine; one or more of these can be maintained in the field of vision, whilst the reagent is allowed to flow in in exceedingly small quantities from the edge of the covering glass. The significance of this mode of preparation will be understood, when it is recollected that in fresh and even in older preparations of blood when thin, any addition of fluid sets the constituents in motion, which is very soon perceptible under high magnifying powers. Under these circumstances, if you wish to know how a certain granule is changed by the reagent, you must endeavour to follow it by means of corresponding motion of the slide; but since the granule is not alone set in motion but the red corpuscles also swim with it, the former is soon covered over by the latter, and the eye and the hand must be taxed to the utmost to follow the object in its course. Even then the same degree of certainty can never be attained as when the body remains quiescent, while the reagent is allowed to slowly act upon it. Especially when there are a number of corpuscles in a plasma-island near the edge of the preparation, and the reagent is allowed to flow in from the same side, the reaction can be perceived as soon as the fluid reaches the blood.

In case I wish to examine for several hours consecutively any one portion of a specimen of any one granule in particular, I build up a wall around the slide upon the microscope. Said wall consists of four pieces of folded, moistened and bibulous paper, which are kept wet from time to time by means of a drop-bottle. In this way there is no trouble in keeping one preparation under the microscope for eight or ten hours.

I have obtained some interesting results by being able to keep my preparations for several hours in succession upon a heated slide. For this purpose I used the heatable object-table, invented by me and described in my *Handbuch der Gewebelehre.* To this table I attached a copper wire about twenty-five ctm. long, and, placing a small spirit lamp under the extreme end of it, gradually brought the latter nearer and nearer to the microscope, until the desired temperature was attained. I then enveloped each end of the slide with moistened blotting paper, so that while the middle of the slide rested upon the metal the ends of the same found steady support upon the hard rubber by means of the paper. Two more folded strips of moistened paper were then placed uprightly on the two remaining sides of the slide, one before and the other behind the objective lens of the microscope. Twice I moistened these strips of paper from time to time; my specimen was protected from drying in spite of the artificial heat. Finally I placed a large screen before the microscope, which concealed the flame of the spirit lamp from the eye of the observer. In spite, therefore, of the loss of light occasioned by the use of the heating apparatus, I was able to work with lenses of high power.

When the number of specimens to be examined prevents us from watching any one of them continuously, as we might wish to do, it is well to have some fine shreds of silk floss, to serve as landmarks, running through the preparations. These are best taken directly from a cocoon and are to be applied to the slide before it is charged with the blood, by laying them across the field and sinking them into the two strips of wax before mentioned. By this means it is very easy to find again any given point, even when using the highest powers, especially if assisted by a sketch upon paper of the bearings made at the first examination.

By the method which I have now described I have in a large number of specimens been able to watch, for several days in succession, the elements which are to be seen in fresh blood, and I am now able to state that those bodies which I have mentioned as due probably to coagulation, or which may be torn fragments of protoplasm, disappear, as a rule, in the course of twenty-four hours. It is, therefore, for our present purpose, immaterial whether my suspicion as to their nature is correct or not; for the only question at present is whether these bodies are identical with those discovered by Lostorfer, or whether the latter are produced from the former; in either case Lostorfer's bodies would be found, especially in the fresh blood, both of sound as well as diseased persons. Such, however, is not the fact. I have, in a series of fresh specimens of blood carefully drawn and registered all that was to be seen in the plasma-islands, and watched the bodies just referred to until their contours had become indistinct and pale, when around them new bodies, not visible before, would spring up, and, on repeated examination, be seen to grow larger and larger, until they finally assumed the unquestionable characters of Lostorfer's corpuscles. In what way the bodies visible in the fresh blood disappeared I cannot explain, but such explanation does not pertain to the question in hand.

The inquiry which I raised at the outset whether Lostorfer's corpuscles are to be found in fresh blood, is now partly answered, but I can relate an observation which disposes of the question still more decisively.

In Prof. Zeissl's syphilitic wards, in the month of March of this year, was a man whose blood was unusually favorable for my investigation. The patient was twenty-two years old, somewhat poorly nourished, and had suffered with syphilis for four months. I took several drops of blood from his vola manus, prepared a number of specimens, and after twenty-four hours found the edges of the latter strewn with the looked-for corpuscles. A second set of specimens showed the same result. On trying a third set, no corpuscles were to be seen the first thirty-six hours, and were only visible at the end of forty-eight hours.

At this time I had just stopped having my room warmed, although the weather was still somewhat cool. On considering the different circumstances which might have occasioned the delay, I was led to take the temperature of the room into account. On the following day I had my room heated up to 22° Centigrade, obtained new specimens, studied them carefully, and noted down all that was to be seen in several portions near the periphery. In the course of three hours the corpuscles had made their appearance in large numbers. Several of them were so large that I was forced to one of the following conclusions: Either, in spite of all my previous observations, I had deceived myself, the corpuscles had been present in fresh blood and had escaped my detection, or their growth in this case had been very rapid, and it must be in my power to bring it under direct observation. On the following day I took again a fresh specimen, had my chamber warmed as the day before, placed a portion of the specimen near the periphery, under Hartnack's immersion lens No. 10, and watched. About fifteen minutes had passed since the drop of blood was taken, when it seemed to me that I could detect, in the otherwise clear plasma-interspace, a few granules. In the course of ten minutes more these granules had assumed so decided a character that I could clearly define their positions. At the same time it appeared to me as if numerous fresh granules were making their appearance in the same interspace. I could not look continuously, since, to spare my eyes, I had to look up from time to time, but I was able to fix the position of certain granules so distinctly that I recognized them at once at each fresh glance. At the end of half an hour the granules which had first appeared were already so large as to enable me to recognize them as the bodies I was looking for. At the end of an hour and a half the first granules had grown to the size of a nucleus of a pus corpuscle, and the whole plasma-interspace was scattered over with similar but smaller granules. There was no longer the slightest doubt; I had before me Lostorfer's bodies; they had made their appearance and grown up in a clear plasma-interspace under my own eyes.

On the following day I repeated the experiment, but, instead of heating my room, I laid the specimen upon my heatable table, warmed up to 25° Centigrade, and repeated exactly the observations made on the previous day. The conclusions drawn from numerous earliar examinations, continued over several days, were now supported by direct observation, and my question was answered with certainty.

Let us now turn to the first and chief question. What is the nature of these bodies? They are either organisms or they are inorganic. From what we have thus far learnt we cannot decide with certainty in favor of either of these possibilities. We know that they grow, but growth alone is not sufficient.

Lostorfer has stated that the bodies bear sprouts. This statement is also true; but I have ascertained by exact observation that some of the sprouts arise not through a process of growth, but through the apposition of other bodies.

When quite fresh most of the corpuscles are spherical and under No. 10, and even No. 15, immersion lens, are seen to be homogeneous. Now it was my good fortune to observe how a small corpuscle, which had been lying for some time in the neighbourhood of a larger one, approached the latter and became attached to it like a button. After a time the contour changed in such a manner that the sharp constriction between the two bulged out and became concave on either side; then the concavities became effaced; the general outline was club-shaped; then the summit was gradually drawn in and the club was changed into a circle. I could, of course, not say that all the bodies provided with sprouts had arisen in this way, but it was enough to have once distinctly witnessed such a mode of origin. It was enough to have shown that the appearance of sprouts already formed proved nothing as to their mode of life.

Lostorfer has also stated that the corpuscles sometimes grow into sack-shaped bodies or tubes. I only once saw a corpuscle with a long, adherent tube, but in this instance I distinctly observed its mode of origin, which was due to the arrangement in a row of three small corpuscles that at the commencement of the observation lay near each other by the side of a large one.

The interspaces or constrictions became effaced, and a homogeneous tube projecting from the large corpuscle was completed. Hence, no inference is to be drawn with regard to their organization, either from the sprouts or from the branches exhibited by the corpuscles.

My observation with regard to the origin of sprouts throws much doubt on the phenomena of growth, of which I spoke above. Having once determined that the smaller corpuscles blend with the larger, why should the smaller ones not grow in consequence of the aggregation upon them of still smaller ones? It is true that the corpuscles, whose growth I witnessed, showed no signs that their growth was due to apposition, but that is nothing to the purpose.

My observations have, therefore, shown that the corpuscles appear in the field of vision at first so small that they can scarcely be perceived. Hence corpuscles must exist of an exceedingly minute size. But further, it is not improbable that they exist at the outset beyond the limits of vision, and only become visible through their growth. What prevents us also from supposing that the corpuscles, while growing before our eyes, grow by the apposition of smaller invisible bodies? It will be objected that we have no example of such growth in any non-crystalline bodies; but do we know of any instance in which an organism grows so rapidly as must be the case when, in the course of an hour, it increases from the limits of invisibility with an immersion lens No. 10 up to the size of a very large nucleus?

It therefore appears to me conclusive, that we can decide nothing as to the nature of the corpuscles from their increase of size. Thus much only is determined, that we have to do with entirely unknown structures.

Meanwhile I had observed another fact which led me somewhat nearer to a conclusion as to the nature of these bodies. I had ascertained that their chemical reaction is different immediately after their appearance upon the heatable table from what it is when they have been developed to a large size by being kept in a moist chamber for several days. In the latter case they are but little affected by acids or alkalies; they shrink a little, but are not destroyed. In an early stage of their existence, however, they are destroyed by acids and alkalies, and even by water. For this reason it is important that the specimen at the outset should be protected from water. When once grown, they will survive for days, even when the blood-corpuscles have been destroyed by moisture. This peculiarity can be readily explained under the supposition that these bodies are organisms. In that case they would have their youth and their adult life; and these would correspond to the early and the late stages of animal cells. In the former their outer layers would be relatively soft and in the latter hard.

If, however, we suppose that the bodies are not living but consist of colloid substances, the possibility that their outer layers may then also become hardened in the course of time, is not excluded.

With these doubtful conclusions, I was now on the point of terminating my investigations with regard to this question, and wanted only to try if a still higher degree of heat would not afford useful information. I had already made the attempt before with the blood of another patient, and the bodies which had gradually appeared had again disappeared through the formation of vacuoles. Now I wished at least to investigate the development of the vacuoles.

Upon warming a specimen of the blood of the above-mentioned patient very slowly and carefully up to about 38° Centigrade, I saw a remarkable sight. A perfectly clear plasma-interspace was in the course of about fifteen minutes so filled with very fine bodies, that I might have supposed that a precipitate had taken place. It was scarcely possible to watch the development of any one body. Their number was too great and they lay too near each other; after necessarily resting my eyes for a moment I could not be certain that I was really looking at the same one as before. One thing, however, was certain, the number of the bodies was gradually increasing, and soon some of them were so large that I could decidedly recognize them as Lostorfer's corpuscles. I noticed with surprise that a large number were provided with bulbous-shaped projections. I now fixed my eye, so long as I could look continuously, upon one of these, and found that the two bodies, the larger and the smaller, soon approached each other and again rapidly became separated. After this observation, which was somewhat trying to the eyes, I was obliged to rest for a short time, and whenever I commenced again I was obliged, for reasons already given, to watch a new pair, of which, as before stated, there were many in the field. I first noticed that the two bodies always had a swinging to and fro motion, as if they were firmly bound together. By changing the light I succeeded occasionally in seeing the thin and structureless thread which bound them together. Finally, I observed most distinctly how two such bodies slowly approached each other, then again separated, once more approached, and once more separated, and finally broke suddenly apart.

The number of the granules apparently still further increased, the field of vision was thickly scattered with them, but they did not attain to a considerable size; they remained much smaller than when more slowly developed at a temperature of 22°–25° Centigrade. In spite of careful observation I did not at this time succeed in observing any union between two corpuscles; although, in many cases, I saw the bond of union between them becoming broader and shorter, so that occasionally the two appeared to be one with a broad crevice upon it. Still even this crevice became broader and narrower, and respectively longer and shorter at intervals.

After these same phenomena had been observed again on the following day in blood from the same patient and under the same conditions, I regarded the chief question as answered. So far as we know, the phenomenon of repeated approach and separation between two bodies can only be explained upon the supposition that their bond of union is contractile and that the bodies themselves are organisms.

The phenomena of growth now assumed greater precision. I have before remarked that, if we observe the running together of two fluid bodies, we may explain the growth in question as an instance of mere apposition. I only stated, however, how I explained the matter to myself, before having further observations at my disposal, and I coerced the circumstances so as to leave no room for a supposition which was open to objections; for in all my observations of the union of two of the bodies I never saw such a running together as takes place between two drops of fluid. When two drops of fluid are lying against each other they may for some time adhere to each other and present the form of a sprout; but so soon as their surfaces at any point are broken, and so soon as their masses become confluent they suddenly form one drop. In the case of the corpuscles this process went on slowly; the contours gradually changed; the form of a sprout was changed into that of a biscuit; then one incision after the other became effaced; and even then upon the upper, or upon the highest surface was a line of separation visible, until finally an oblong form was established without trace of division. In this oblong form, the bodies continued for some time longer. I saw repeatedly that facets were even at this time formed and then became effaced. The final result of all these changes was a globular form, with which the motions ceased. I was not willing to regard these slight changes of form as indications of life, because I could not observe the disappearance and reappearance of distinct forms. I was well aware, however, that these changes of form are frequently present in protracted processes of division. It was also just as probable at least to suppose that two bodies lying near each other, and that suddenly approached each other, were bound together by a contractile thread, as that they were approximated by mutual attraction.

In the processes of division to be seen in the tissues of a frog, when portions of the same have been laid bare or have been cut out of this animal, I have often seen how two halves of one cell separated from each other until they were only united by a thin thread; how the thread sometimes changed its optical characters, until it became invisible; how the division was then apparently complete, and yet the products of division again approached each other, and gradually united. All experienced pathologists are also conversant with the fact, that pus corpuscles, so long as they are exposed to certain conditions of division, vegetate in such a manner that they do not attain any considerable size. On the contrary, when the conditions are unfavourable for division, their growth is considerable, and they then begin to form permanent tissue. This observation corresponds in every respect with the action of our corpuscles, which remarkably increased in size at a temperature of 22–25° C., while at the temperature of 38° C. they grew less, but increased in number instead.

We have, therefore, besides the appearances observed on warming the preparations, a noticeable series of indications in favour of the supposition that the bodies are organisms; indications which are of themselves not sufficient to prove the supposition, but which fall in with it exceedingly well.

We have to do with organisms which are developed only very slowly at a temperature below 10° C., and at a decidedly lower temperature perhaps not at all; at a temperature of 20–25° C., their development is somewhat rapid, and is still more rapid at the temperature of the human body. When they have once attained a certain size, they then remain in the form of globules or discs (in many cases it was not possible for me to make the differential diagnosis), either in a state of rest, or as dead bodies; they can then be kept for days, even when the preparation of blood is destroyed by moisture.

These organisms increase by sprouts or by division. The outgrowth of large threads or filaments was not, however, observed in any place, or under any of the conditions to which I subjected them.

Their development is essentially favoured by the neighbourhood of a large number of blood-corpuscles, and again, by the neighbourhood of air bubbles. They will usually first be found where a plasma-interspace is bounded on one side by air, and on the other side by a thick layer of blood-corpuscles. When the preparations are put up in the manner above described, and are first examined over their entire margin, the bodies can scarcely be overlooked, if they make their appearance at all.

When they have grown under my own eyes, they have very decidedly spread from the margin, and, therefore, from the confines of the air towards the centre of the preparation. Still it is not to be understood that I have seen how the bodies advanced. I have on the contrary observed that the finest granules always begin to be visible at the same time in one zone; and first in a zone in the neighbourhood of an air bubble. In a short time, the whole of this plasma-interspace would be filled with them, and only afterwards could I perceive analogous appearances in the interspace the next farthest removed.

With regard to the sources from which these organisms proceed, we have the following alternatives. They arise either from the air or from the blood; and in the latter case their germs already existed in the circulating fluid, or they sprung up subsequently by spontaneous generation.

The last portion of the alternative will indeed find few advocates, and I myself would not be included among the number. But nobody will deny that in this case *generatio æquivoca* must come into consideration as one of the possi bilities; for so soon as we have gone so far as to assert, that organisms arise from germs which are not to be seen with the best microscopes in existence, we assert something which cannot be proved, so long as the possibility of *generatio æquivoca* is not excluded with all imaginable certainty. *Generatio æquivoca* is not proved, but its possibility is not out of question.

In case the germs arise directly or indirectly from the air, we have perhaps to do with organisms which are well known under other forms, perhaps with bacteria, which finally appear in every preparation that is allowed to stand. Only we must then suppose that they assume in the blood of certain men, a mode and form of development, which, so far as I know, have never hitherto been discovered.

The fact that their development first begins where the plasma borders upon a layer of air, is perhaps in favour of their arising from the air. But it is also to be borne in mind that perhaps the air in the neighbourhood only favours their development; and this is the more worthy of consideration, since I have found that the development proceeds most favourably when the layer of blood, bounding the plasma-interspace upon the opposite side, is somewhat thick.

Let us now suppose that these newly known organisms arise neither directly nor indirectly from the air, and also not by spontaneous generation. We must here consider, that in the blood of one individual, derivatives from the cells of this individual or of other similar individuals may circulate, which the first obtained from the secretions of his parents or through contagion from one individual to another. The supposition that such derivatives form cells, which in consequence of their smallness are not visible under the best microscopes, is indeed entirely hypothetical. It must, however, be confessed that the existence of such is not at the outset to be excluded.

I turn now to the second question, viz., whether the organisms in question are present only in the blood of syphilitic persons? This question I must at the outset answer in the negative.

I have examined, in all, thirteen cases of general syphilis, and in these I found the bodies in nine cases in large number; in two cases they were certainly not to be found, and in two cases they were so few that for reasons hereafter to be given I was obliged to regard the result as negative. Among the cases which I have examined, we have, therefore, nine successful and four unsuccessful.

At the commencement of my investigation I found no trace of these bodies in ten cases of healthy, well nourished individuals, in one case of pneumonia, one of heart-disease, four cases of typhus abdominalis, one of typhus exanthematicus, and ten cases of smallpox.

The first case of lupus which I examined showed them in considerable number, and in nine more cases of lupus I found them once again; so that they were present in two out of ten cases of lupus.

After this experience, the matter, as Lostorfer had represented it, appeared to gain in probability. I had found then out of thirty-seven non-syphilitic persons only in two, and in these two cases the supposition was allowable that between lupus and syphilis there is no absolute distinction.

In the meanwhile, a circumstance occurred which turned my attention in a new direction. The patient above mentioned, whose blood afforded me an opportunity to observe the development of the corpuscles in so short a time, had several pretty severe attacks of hæmoptysis. Since I had observed the appearances which I have described in no other case so well marked as in this, the idea that deficient nutrition either alone or in combination with some other general disease, might have an essential influence in the production of the bodies, forced itself upon me. I therefore determined to examine the blood of persons suffering with severe chronic affections. My examinations were, indeed, few in number, but they were amply sufficient to enable me to decide the question. I found the bodies in large numbers in one case of carcinoma ventriculi, and in two cases of tuberculosis, and in all three cases on the second day of keeping the specimens. Twice I also found them in moderate numbers in another case of advanced disturbance of the nutrition, in which Bright's disease was combined with disease of the heart; and again in a case of anæmia following variola, the question appeared to me to be settled so far as was desirable for the present.

The investigation of these cases justified the decision that the organisms we have described are not exclusively to be found in the blood of syphilitic persons.

When we, however, consider that I failed to find the corpuscles in the blood of a very considerable number of persons, a portion of whom were healthy and the remainder affected with acute diseases, we cannot deny that Lostorfer's discovery is of some consequence in the pathology and especially in the doctrines of syphilis.

I will not assert that these bodies are not present in the blood of sound persons, and of persons affected with acute diseases. The number of cases which I examined is much too small for such an assertion. Thus much, however, is evident from my observations, that these new elements of the blood are present —seldom in sound persons and in persons with acute diseases—but very frequently in persons suffering from long continued chronic disturbances of the nutrition, and in persons affected with syphilis. We must now wait for the results of farther investigations. Should it be proved that my observations are correct, or nearly so, when applied to a large number of cases, I may have something further to say about the facts.

In conclusion I wish to show that the possibility of a very close connection between Lostorfer's corpuscles and the appearances included under the name of syphilis, is for the present not to be rejected. This will chiefly depend upon the question, whether the germs of these bodies were or were not previously present in the blood. If the latter be true, then indeed the supposition of such a near connection will be little justified. For it will then be more natural to suppose that there exists in the blood of certain persons a chemical condition, which exerts a favourable influence for the development of organisms coming from without. Should it, however, appear that the germs exist in the blood, then we shall have reason to suppose that organisms under certain conditions may assume certain peculiarities, and also among others, those peculiarities which we ascribe to the syphilitic virus.

These considerations will also be justified if larger statistics should prove incontestibly that the organisms are really present in the blood of all men who are affected with long-continued disturbance of their nutrition. For there is nothing against the supposition that the syphilitic virus may be generated in man under conditions which are unknown to us; that long-continued disturbance of the nutrition establishes such conditions, and that the virus first assumes its peculiarities under certain conditions, perhaps when it passes in single or manifold descendence from one individual to another.

The circumstance, that these bodies were found in large number precisely in cases of syphilis, carcinoma, and tuberculosis, is surely not opposed to such an assumption.

The circumstance, that in all three diseases we meet with a series of wholly unexplained phenomena, should stimulate us to follow every track that may lead to an explanation so long as it is scientifically justified.

Finally, the circumstance, that my numerous and careful investigations of the blood of syphilitic persons were fruitless in every other direction than that stated by Lostorfer, may serve to further test his discovery until we can obtain light as to its value.